

THE
BOSTON MEDICAL AND SURGICAL JOURNAL.

VOL. XXXIX. WEDNESDAY, OCTOBER 11, 1848.

No. 11.

CONTRIBUTIONS IN PHYSIOLOGY BY DR. PAINE.

THE CIRCULATION.

[Continued from vol. 38, page 527.]

To the Editor of the Boston Medical and Surgical Journal.

SIR,—At the close of my last communication, I remarked that, "at another time I will briefly continue this subject, for the purpose of showing that mind is a perfectly distinct essence." Since then, I have concluded to examine the question in the form of an essay, and will now, with your permission, extend my remarks, as embraced in the foregoing article, upon the subject of the circulation in plants.

Absorption by the roots is considered an inadequate explanation of the circulation of sap among those who advocate the doctrine of capillary attraction; and, if numerical strength were to decide the question, what I am about to say would be a waste of your valuable Journal. To interpret the process in plants, the leaf, or its equivalent, has been taken as especially instrumental; serving either as an exhausting apparatus by evaporation, or under the designation of endosmosis, or contributing its aid by supposed chemical influences, through the operation of light, upon the ascending sap. Some one of these hypotheses is considered an indispensable auxiliary to the doctrine of capillary attraction as applied to the circulation of sap.

An ingenious application of chemical principles has been propounded to satisfy the supposed exigencies of capillary attraction as concerned in the absorption and ascent of sap, and professes to afford the true solution of the downward motion. This hypothesis is also thought to be a new obstacle to the doctrine which ascribes life to a plant, and the dependence of its circulation and unique products upon vital actions. The hypothesis derives, also, no little importance from its application to the circulation of the blood, and the admission that, if it cannot be sustained in respect to plants, it must be equally groundless in regard to animals. The doctrine comes recommended to our attention by its distinguished advocates.

I have already endeavored to show, extensively, the want of all foundation for the more comprehensive principle set forth by Liebig, that—

"The CAUSE of the state of MOTION is to be found in a series of

changes which the food undergoes in the organism, and these are the results of processes of decomposition, to which either the food itself, or the structures formed from it, or parts of organs, are subjected."

This summary principle, in which oxygen gas figures conspicuously, is the combustive doctrine of life. The projector held it to be applicable to every motion and to all the phenomena of living beings, in health and disease, and even in death. It was also made to explain our very thoughts and passions; these being imputed to the union of oxygen with the combustible elements of the brain.

I cheerfully conceded that—"this summary principle, were it true, would be truly beautiful." I therefore felt the importance of showing that "it was not only deficient in every necessary element, but was contradicted by all the phenomena of sympathy and by all that is known of pathology and therapeutics; and this, too, according to the author's own repeated and fullest affirmations and doctrines to the contrary." This I endeavored to execute particularly in my articles upon Digestion and Animal Heat; and I am thus provided with a vast series of facts in advance, which must be taken in connection with what I may now say of the corollary from the fundamental doctrine. This corollary consists in the application of the general doctrine to the circulation of the sap and the blood. It supposes that the movement of the sap, upward and downward, is generated in the leaf by the action of light in promoting the decomposition of carbonic acid gas. The imperfect ascending fluid is thus converted in the leaf into perfect sap, and the change is supposed to institute a propelling force in the imperfect juice, by which the elaborated sap is driven out of the leaf and through its downward course. The force, generated in the leaf, is also considered, from the motion which ensues in that part, as the most essential cause of the ascent of the sap, or that the fluid is thus *lifted* from the roots to the summit of the most lofty trees.

The same principle is applied to animals. The pulmonary circulation is said to depend upon the union of the oxygen of the air with the carbon of the venous blood, in consequence of which this blood drives the decarbonized into the left auricle. But in the case of the systemic or greater circulation, the order of things is reversed; for here the motion is supposed to be generated by the union of oxygen with the "structures formed out of the food." The same order of events obtains in the liver—all referable to "a series of changes which the food undergoes in the organism," &c.

I shall now limit my remarks to the supposed condition of the circulation in plants; since, if the hypothesis can be contradicted here, it must equally fail, as admitted, in respect to animals; and I shall endeavor to avoid whatever I may have hitherto said. Perhaps, however, it may be well to state the confidence which is entertained in the equal applicability of this theory to plants and animals, and the admission of its necessary failure throughout should it appear defective in relation to either of the organic dominions. This has been forcibly, as well as beautifully and justly, expressed by my learned friend and colleague,

Professor Draper, in his able work on the *Organization of Plants*. Thus—

“As we have said, it is the character of a true theory to be applicable to all cases, and to render a clear account of every circumstance that may arise. A true theory is like a window of crystal glass, through which we can see all objects in their proper position, and colors, and relations, no matter whether they are such as are near, or those that are at a distance; no matter whether they are directly before us, or enter only obliquely into the field of view. A fictitious theory is like a Venetian blind, which has to be set in a certain position with respect to the observer, and only shows him objects for which it has been adjusted, and these in an unsatisfactory manner; but if he moves to one side or the other, or endeavors to see objects which are not directly in his way, his view is intercepted, or, perhaps, unless he makes a new adjustment, the light is shut out altogether.”

In the first place, then, it appears to me that the hypothesis contains a fatal element—the prodigious amount of force which is said to be generated in the leaf, as well as the lungs and other soft structures of animals. On *this point* I am bound to abide by the decision of the chemists, who say that such *must* be the consequence of the chemical changes which are supposed to be in progress for the production of motion. As expressed by these philosophers, who designate it as “an *inexpressible force*,” it would be abundantly sufficient for any purposes in artillery or in blasting rocks; and I infer, therefore, would hardly be withstood by the leaf or the lungs.

In the next place, there are many other circumstances attending the circulation of plants, as well as animals, which it would not be easy to interpret by the chemical doctrine, but which are readily explained by the vital. Such, for example, as the remarkable vigor of the circulation in the *acer saccharinum* before there is a development of the bud. Indeed, the harvest of maple-sugar often takes place in the Northern States while the earth is covered with snow to the depth of many feet. The circulation, too, is most vigorous after frosty nights succeeded by warm mornings; and when the temperature of the air for a night or two rises to some 40° F. the flow of sap is often nearly or quite suspended, but restored in profusion on the return of frost. What in chemistry will explain such a phenomenon? And if it retreat before obstacles of this nature, must it not abandon the whole ground? Nay, how palpable the force of a single fact, when it is considered that the phenomenon is due to the effect of heat as a vital agent on the irritability of vegetable life; and whether operating at the higher and more uniform degrees, or alternating at the freezing point, the exact explanation is involved in the law of vital habit, as set forth in the *Institutes of Medicine* at page 363—370.

Such, mainly, is also true of the vine, which was the subject of many experiments by the celebrated philosopher Dr. Hales, as appear in his *Vegetable Statics*. These experiments are allowed to have been ably and critically conducted, and are standard references. Let us, therefore, interrogate some of these experiments, and see how far they correspond

with Nature, or how far they contradict her and bear out the chemist. Now, in some of his experiments there was not only an absence of leaves and buds, but the stump alone was the subject of observation. There was wanting, therefore, the fundamental requisite of the chemist, and, indeed, I may say, what is considered indispensable by all the physical philosophers to the simple doctrine of capillary attraction as it regards the ascent of sap. Take as an example *Exp. xxxvi.* Thus—

“April 6th, at 9, A. M., I cut off a vine on a southern aspect, two feet nine inches from the ground. The remaining stem had no lateral branches. It was seven eighths of an inch in diameter. I fixed on its top the mercurial gage;” of double curve to admit the flow of a few inches of sap.

For several days the mercury was more or less pushed up by the sap, according to the state of the weather.—“April 14th, at 7, A. M., the mercury rose to 20 inches high. At 9, A. M., 22 inches. Fine warm sunshine. Here we see that the warm morning gives a fresh vigor to the sap.”—“April 18th, at 7, A. M., mercury 32 inches high, and would have risen higher if there had been more mercury in the gage. From this time to May 5th, the force gradually decreased [the life of the plant giving way]. On the 18th of April the force of the sap was equal to 36 feet height of water.

“Here,” the Doctor concludes, “the force of the rising sap in the morning is *plainly owing to the energy of the root and stem.*”

In another and similar experiment, at the same time (“the mercurial gage being fixed *near the bottom* of a vine) the mercury was raised by the force of the sap 38 inches, equal to 43 feet+3 inches+ $\frac{1}{2}$ height of water; which force is near five times greater than the force of the blood in the great crural artery of a horse; seven times greater than the force of the blood in the like artery of a dog; and eight times greater than the blood’s force in the same artery of a fallow doe”—as ascertained by the rise of the blood in long glass tubes.

In these experiments it is sufficiently manifest that all the physical hypotheses fail, since all of them assume that the leaf or its equivalent is indispensable to the progressive rise of the sap. The result, I say, shows, what the observation of all organic nature teaches, that so important a function as the circulation, and so exceedingly variable as in plants, yet most exactly suited in every species and every individual, but varied in all the species, to the methodical steps in vegetation, is not left to the capricious operation of any chemical or physical agencies, and that a force is established at the very base of a plant, that shall not fail of the exigencies of vegetable life according to its progressive changes. It follows, therefore, that the sap is moved by something peculiar to living beings, and this is called a vital action. The motion which we have seen, however, would prove utterly destructive to the leaf, and even to all delicate branches, without a gradually countervailing intervention of that action, since the subdivision of vessels will not explain the diminution of force. I hence infer, what is denoted by other important facts, that the reduction of force arises, also, from a modified action

in the vessels leading to the bud, as well as in the bud, or leaf, itself. Here a new action is set up, and a new motion of the sap begins, which is propagated along its downward course by a universal action of the vascular system, modified in different parts according to the special final causes of each part. Nor is it true, as supposed by one of the ablest chemists of the day, that, "Physiologists have here stepped in with their phantom Vitality, and *explained* the descent of the elaborated sap on visionary hypotheses, *that it was alive, or had obtained some vital qualities.*"

There having been no leaves in the foregoing experiments, and, indeed, only a short stump of the vine, the results were not unexpected to the philosopher, who adopts the theory that the circulation of sap is owing to temperature. But temperature would not always explain the phenomena, capillary attraction was little understood, and chemistry was yet unfledged. Accordingly, as in all cases where genius departs from Nature, even the acute mind of Dr. Hales has a special hypothesis for each apparent difficulty; sometimes borrowing from the theory of the vitalist, and actually raising hypotheses in direct opposition to each other. Take the following examples where the leaves had obtained their full development. Thus—

"July 4th, at noon, I cut off within three inches of the ground, another vine on the south aspect, and fixed to it a tube seven feet high, and filled it with *water*, which was imbibed by the root the first day, *at the rate of a foot in an hour*, but the next day much more slowly; yet it was continually sinking, so that at noon day I could not see it so much as stationary;" the life of the stump now giving way.

Here are two important facts. There was no apparent upward force, though there may have been some mingling of the sap with the water; and, secondly, the water being vitally adapted to the plant, it was actually carried down to the roots, at the rate of a foot an hour from the tube. There was no chemistry here to effect or in any way influence the descent. The pressure was also slight, and the hypothesis of the laboratory probably scouts the notion of gravity. Besides, I may say, if gravitation effects the descent, it should *prevent* the ascent. This experiment, like the preceding, appears to be decisive against our rival friends, and in a more important aspect; for the *descent* of the sap had been a greater problem to them than its *ascent*. The importance and compass of the proof will be at once perceived. But he who made the experiment, seeing the want of agreement with the preceding, fancied, like a great many other philosophers, that a conflicting fact would justify a special hypothesis. Let us, therefore, hear the Doctor upon this troublesome point. Thus—

"Now, since the flow of sap ceased at once, as soon as the vine was cut off the stem, the principal cause of its rise must, at the same time, be taken away, viz., the *great perspiration of the leaves.*"

That is the doctrine, along with capillary attraction, of a large section of the physical school. But it arises from a defective observation of facts and from an ignorance of the difference in the physiological con-

dition of the vine before and after leafing. Indeed, how can any *such* distinction be appreciated by those philosophers? In the experiments first recited, the vine was in its budding, and, therefore, bleeding season; in the present, the leaves had become developed; and notwithstanding the lifting power ascribed to them by the chemist, and that from the evaporation imputed by others, the circulation is transcendantly greater in the budding season, and Hales might have amputated the largest limb, with all the other leaves remaining, and the same descent of the water would have occurred, and prompted a different hypothesis.

And now contrast the foregoing experiment with his conclusion as expressed in *Exp.* xxxviii.; the words in Italics being designed by myself to facilitate the hasty reader. It is a hypothesis, directly opposed to the preceding, for the purpose of expounding another fact.

"The sap," says the Doctor, "begins to rise sooner in the morning in cool weather than after hot days; the reason of which may be, because in hot weather much being evaporated, it is not *so soon* supplied by the roots as in cool weather, *when less is evaporated.*" In *Exp.* xlv. he says, "it was found that the trunk and branches of vines were always in an imbibing state *caused* by the *great* perspiration of the leaves, *except in the bleeding season.*" At that season the problem of the stump led him to conclude that "the force of the rising sap is plainly owing to the *energy* of the root and stem." (See *Exp.* xxxvi.) Will the chemist explain?

In one of his experiments he attributes an effect to the "sun's warmth" in making the vessels "*dilate and contract a little.*" This is what he means by "the *energy* of the root and stem." Had he adhered to that explanation, he would have had no difficult problems to expound, no conflicting experiments, no contradictions of himself. Few philosophers, however, have been as accurate in their experiments, and few better qualified to reason upon facts, than Dr. Hales; and what may we not, therefore, conclude of the reasoning of those who have assumed spurious inferences in vegetable physiology as a groundwork for the circulation and other organic processes in animals?

Thus it ever is with all who depart from their main field of operations to build up the difficult parts of other sciences. Hales was a divine, and although adroit in experiments, and better qualified by impartial habits than the chemist, it is no detraction from his (or their) exalted merits to say, that he knew so little of physiology he was incapable of applying or even perceiving the facts which the student of organic nature may readily seize and convert to the philosophy of life, and turn against the conclusions of the original observer.

Am I not, therefore, entitled to conclude, that organic beings are contra-distinguished from inorganic by what is popularly known as life, or vitality, and with the summary remarks of the greatest scientific botanist of the age, of one who has devoted his life to the subject, Professor Lindley of the London University, as expressed in his able analysis of the "First Principles of Botany," that—

1. "The movement of the sap depends upon a vital irritability, and is independent of mechanical causes."

2. "The proximate principles are formed by the vital powers of the plant acting, in conjunction with air and light, upon the fluids contained in its system."

3. "*All the phenomena connected with the growth of plants are caused by an inherent VITAL ACTION.*"

New York, Sept. 25, 1848.

CASE OF DISLOCATION OF THE SHOULDER, OF FORTY-TWO DAYS
STANDING, REDUCED BY DR. JARVIS'S ADJUSTER.

BY G. O. JARVIS, M.D.

[Communicated for the Boston Medical and Surgical Journal.]

THE above is selected from a number of cases, some of longer, some of shorter duration. This is one, however, which may very justly be regarded as a fair exponent of the whole, except so far, perhaps, as there may have been any reason to fear, that in *this* the axillary artery was implicated.

It is not, however, for the purpose of showing "what has been done" that this case is presented for publication; but it is to "show how it should be done." Nor is it that another instance may be given, where through much tribulation, or some not easy to be accounted-for, yet lucky effort of our own, our labors have happened to be crowned with success; but that what may be regarded as the *true*, the *only* true principles, those on which we can with some confidence rely, may be so clearly set forth that they shall, at once, become obvious to all who will give themselves the trouble to examine the subject.

A further object is to show (not only by this case as an example, but by the philosophy which the subject itself necessarily involves) that the pathology of the two stages of a luxation, *recent* and *old*, is as distinct, and that they require as distinct and different modes of treatment to reduce them, as the several stages of typhus fever differ from each other in their pathology, requiring such a diversity in the mode of treatment to fulfil the various indications as they arise.

A still further object is, that what is regarded as the *correct mode* of using the adjuster in old dislocations may be very plainly shown. The importance of this, I trust, will not be questioned, when it is considered that it would not be strange should more than one be found of the *hundreds* who now own the adjuster, who either do not understand its use, or do not, from some cause, choose to apply it correctly, even in this particular kind of dislocation; some, perhaps, for want of time, others for want of attention, and others because the instrument itself involves principles so entirely different from those which they have ever been in the habit of using (whether true or false, it matters not), that they cannot seem to comprehend anything which would appear to reflect on those "long-cherished virtues" of surgery, or which would in any way place in the

back ground *their own peculiar* notions. For those, however, who *will* bestow their time, who *will* give their attention, and wish to understand its use, in such cases, for the sake of the good service which they may possibly thereby render in their profession, and also for those who may wish to examine the philosophy of reduction conducted on those principles, that they also may have an opportunity to practise it, I have penned this short article. But especially have I done it, that I might call attention to this much-neglected department of surgery. No department has received so little attention, and none has made so little progress, as what may very properly be called *osseal* surgery; while in fact, in point of real importance, both to the profession and the world, it is not surpassed by any, if indeed it has its equal.

It would be but an act of justice to notice, in this place, a case in many respects parallel to the one about to be related, which was reduced by the adjuster, and published in this Journal under date of March 17th, 1847, in vol. xxxvi., p. 145. The operation was performed by Prof. J. F. May, M.D., of Columbia College, and reported by Robert King Stone, M.D. The operation reflects great credit on the surgeons who performed it, since Prof. May and his reporter appear first to have designed their plan (which certainly was very correct) and then to have carried it out as designed, with very great skill. We would refer the reader to that case, as well as to the one which we shall now relate, as fully corroborating the views which we are about to advance.

During a recent visit at the South, and while I was in Mobile, Ala., I was invited by Dr. Lewis, a resident physician there, to visit the Marine Hospital of that place, of which he was physician and surgeon, for the purpose of reducing a dislocation of the shoulder, of forty-two days standing. Accordingly, on the following morning, in company with Drs. Levert and Carter, of that place, I proceeded to the Hospital to examine the case, and, if judged practicable, to reduce it. A very tall and rather spare man was shown to me, as the subject of the injury in question; yet, notwithstanding his leanness, it was obvious he had good general health, and possessed much muscular strength. I judged him to be about 45 years of age. On examining the shoulder, the head of the humerus was found resting in the axilla, partly under the pectoralis minor muscle anteriorly, while posteriorly the head rested on the axillary artery, so that there appeared to be nothing between the fingers and head of the bone, except the artery and common integuments. The artery was felt to pulsate strongly between the fingers and head of the bone, and was but slightly moveable on the head. The arm had acquired great freedom of motion, equal to what I have sometimes found in the same kind of dislocation, of at least six months standing. Attempts had previously been made in the Hospital to reduce it, but without any success.

Here some questions arose, with regard to the practicability of reduction; the most important of which was—Are there any adhesions formed between the head of the bone and the artery, which would endanger the life of the patient by reduction? Although, from some cause, the artery could be made to move but very sparingly on the head of the bone,

still, from the uninterrupted current of blood constantly flowing through the artery, as could be determined by the feel, I was led to the opinion that there could not be formed any greatly condensed cellular tissue, uniting together the artery and head of the bone, so as to endanger life by reduction, provided it could be accomplished without using violence to the parts around the joint. This opinion, I believe, was also entertained by all the gentlemen present. As it regarded the great *mobility* which the limb had acquired, although it was such, as had, with other causes, defeated all previous efforts, and would undoubtedly defy all the means in common use, still I was inclined to the opinion that by the adjuster a *momentum* might be communicated to the head of the bone, *by the muscles of the arm*, which would disengage the head, and re-open the capsule, through which, doubtless, it had escaped; and thus force it back again to its normal position within the glenoid cavity. On this point, however, none present could consistently express an opinion, having never seen the adjuster used, but all thought so favorably of it, as to desire that the instrument should be tried. Here a medical friend present (who, by-the-by, is quite clever in his profession) suggested that he could reduce it by his heel in the axilla, he having much confidence in that mode, especially since it had not been tried in the present instance. I therefore requested him to make the attempt. He, however, declined, saying he preferred to see me use the adjuster, rather than to have it reduced in any other way. I assured him that he would have that opportunity after he had made every effort in his power, for that I did not believe it in the power of *any* man to reduce it, by any of the old or ordinary means. He still entertained the opinion, however, that it could be so reduced. I then insisted the more strongly on his making the attempt, assuring him, now that he entertained that opinion so decidedly, that I could not consent to even apply the instrument until the effort was made. Seeing, probably, that I was determined on this, he accordingly made the attempt, calling to his aid, of those persons present, just such individuals as he chose to assist him. Tart. antim. had been given the patient, and was occasionally repeated during the whole operation, but, if I recollect rightly, not so as to produce sensible nausea at any time. If I may be allowed to be any judge of such matters, I can assure the reader it was not "all boys' play." After having persisted in the effort (how long, I know not, but until all hands were well fatigued), it was again given up as a bad job; and the case was handed to me to try the power of the adjuster.

The instrument being arranged, it was applied according to instructions given in Jarvis's Lectures, fourth edition, pp. 62, 63, 64. It was then stated, that in all probability, at least one full hour would be occupied in making extension, &c., before even an attempt would be made at reduction. (Reasons for this will appear hereafter.) Extension was now begun, and was increased progressively, slowly, and from time to time, as the muscles were found to yield to the extending force of the instrument, until it was judged that the head of the humerus was extended fairly beyond the line corresponding to the plane of the glenoid

cavity. We were thus occupied in applying the extending force, and in manipulating the limb, the more effectually to disengage the head of the bone, about one hour. It is at this point, then, at which we are to arrive, before even an *attempt* be made at reduction. It being therefore judged that we had now reached that point, the effort was made in the following manner. Dr. Levert, being requested to assist in the operation, placed a silk handkerchief high in the axilla; then seizing the two ends, he forcibly drew the head of the bone outward and a little upward. In this he used all his force to bring the head of the bone out from the axilla, while the arm and instrument was being carried forcibly downward and forward on the chest, so that the arm was made to press hard on the ribs. At this juncture the catch was raised from the ratchet wheel, so as instantly to let fly the whole force of the instrument; thus driving the head of the humerus forcibly upward by means of the *muscles of the arm*. On examining the shoulder, the dislocation was found not to be reduced. No time was, however, lost in applying again the same degree of force to the limb which had once carried the head beyond the plane of the glenoid; indeed, it was a little increased, since it could now be done very readily, lest possibly we might have been mistaken before, in supposing the head to be beyond that plane, while in fact it was not. The arm was again carried forward and downward, as before, while Dr. Levert again assisted, as previously, in throwing the head of the bone outward and a little upward. The catch was again raised, and let fly the whole force of the instrument with the rapidity of lightning. On examining the shoulder this time, it was found to be "all right." All present appeared to be well pleased with the operation, and the result, and probably none more so than the patient.

I beg, here, to be allowed to comment briefly on the foregoing case. Ether was not used, and for the reason that it had not been employed in any previous attempt. Bleeding and the warm bath were not resorted to, and for the same reason. I was resolved, at the commencement, that it should not be claimed that I had derived advantage from any of the ordinary means which had not already been employed. No doubt some advantage might have been obtained by employing either of them; although I confess I generally care not, if the head of the bone can be carried beyond the plane of the glenoid without too much violence being used, how strong the muscular contractions may be. The stronger they are, the more sure am I that the head, by this process, will be driven back through the capsule, without which, reduction can never take place, i. e., in the state in which old dislocations are usually found.

From the little experience which I have had, I am convinced that there are hundreds of cases of dislocation now existing, which might, by the foregoing process, have been readily and safely reduced, but which may have defied (at the time), all of the older and more common means, used *secundum artem*, and to have persisted in them would even have cost the lives of many of the patients. I cannot but indulge the hope, that in the following remarks this fact will plainly appear.

1. *Reasons for adopting the foregoing mode to reduce old dislocations*

of the shoulder.—In employing this mode, I refer to those cases only, where the head of the humerus lies below the glenoid cavity, which, in fact, includes them all, except two, one only of which is classed by writers on the subject among the ordinary dislocations of that joint, to wit, that in which the head of the humerus is thrown forward against the coracoid process, resting on the margin of the glenoid cavity. In this evidently the head of the bone does not lie below the glenoid, and of course the foregoing would be a wrong application of force. The other kind of dislocation to which I refer (and only one case of the kind have I ever seen) is where the head of the humerus is thrown directly backward, resting on the neck of the scapula, under and against the spine and acromion process of the scapula. I should, however, generally apprehend no great difficulty in reducing such a dislocation, provided force be applied in a right direction; but to apply it either in this or in the one just referred to, as it was in the case of the man at the Marine Hospital in Mobile, would manifestly be a wrong application of power; while in all cases where the head lies below the glenoid, it would be a right application of that power.

Before entering fully into the reasons, &c., referred to above, it may be necessary to take a practical view of the anatomy of the shoulder; its bony articulation, the texture of its ligaments, and the arrangement of its muscles, together with the particular kind of mechanical effect which these various parts may have on each other in their several abnormal relations. As to its articulation, then, suffice it to say, it is by ball and socket; the ball relatively very large to that portion of the socket made up of bone. Independent of that security which the muscles, proper to the shoulder, give to that articulation, by being attached above and below the joint, thus passing over it, together with the additional security which it derives from the atmospheric pressure which surrounds it, several *strong ligaments* are so disposed over it, as to admit of motion the most free of any joint in the body, and yet securely to tie the bones together, so as under no ordinary movements to be displaced by this great liberty of motion. The most important of these ligaments, especially in their relation to our present subject, is the *capsular*. It surrounds the joint, is attached in its whole circumference to the margin of the glenoid cavity; reflected from thence over the head to the neck of the bone, is attached to it; surrounding, in like manner, both the head and the neck. A further fact in regard to this ligament, let us here notice; its texture is *fibrous*, its fibres lying parallel, and running longitudinally over the joint.

We will now turn our attention to the *myology* of the shoulder; and in looking at that articulation we shall find it surrounded by twelve muscles, each of which is attached, at some point, above or beyond; passing over it, is again attached at some point below the joint. We shall also find them distributed in the following order: three to pass the joint on the top of the shoulder—to wit, the deltoid, the long head of the biceps, and supra-spinatus; six to pass it on its posterior and inferior surface—to wit, the infra-spinatus, the sub-scapularis, the two teres, minor and major,

the latissimus dorsi, and long head of the triceps ; and three also to pass it on its anterior and inferior surface—to wit, the pectoralis major, the coraco-brachialis, and the short head of the biceps. All of these twelve muscles, except the three first, viz., the deltoid, the long head of the biceps, and supra-spinatus, it will be seen, when the arm is a little thrown out from the body, and *traction* applied to it, tend to cast the head of the humerus downward, and to fix it below the centre of the glenoid cavity ; and it is only while two, at least, of those muscles which pass on the top of the shoulder, viz., the deltoid and supra-spinatus, possess their full power to act, that the head, under the influence of such traction, would be likely to find the glenoid cavity, as the centre of those opposing forces. Let it be here observed, that in a dislocation of the shoulder, where the head lies below the glenoid, the long head of the biceps can have little or no effect (mechanically) on the head of the bone, to elevate it, from the fact, that the head, by being driven through the capsule, becomes disengaged from it, and the only influence which the tendon of the biceps can hold over the head of the bone, to elevate it, being through this capsule, all of such influence necessarily becomes lost to the head of the bone. Under any circumstances, therefore, in such a dislocation, the deltoid and supra-spinatus muscles are the only ones which tend to elevate that head.

Now let us view these facts in reference to our practice. In all *complete* luxations of the shoulder, the capsular ligament must necessarily be ruptured or torn ; and through this rent, must the head of the humerus escape from the glenoid cavity. The deltoid and supra-spinatus muscles are put largely on the stretch, and so must remain, ultimately losing all power to contract, becoming paralyzed by long-continued over distention, until that head is again restored to the glenoid cavity, when the tone of the muscles may be restored and the rent healed. That rent, also, be it observed, would, most likely, be a longitudinal one—since those fibres are more likely to be separated or torn asunder, than they are to be broken across ; or, in other words, any round substance, as the head of the humerus, applied with force against the inside of this ligament, so as to rupture it, would be much more likely to make a longitudinal rent than to make a round hole. A moment's reflection will doubtless satisfy the anatomist of this ; but should any one doubt it, let him try the experiment on the dead subject. This laceration may be a very free one, or it may be only sufficient to allow the head to escape, and indeed a little stretching may have been done even at that. Now suppose, in an adult but young and healthy subject, the head of the humerus should be thus driven through the capsule and lodged in the axilla, and it should there remain for five weeks or more. Within that period of time, by the reparative process which nature sets up, to repair as much as possible the injury done to the parts, the capsule has become healed, and the lacerated part again united, except so far as the neck has served to keep them asunder. The neck of the bone thus becomes closely embraced by the capsule with the head on its outside. The deltoid and supra-spinatus muscles, too, have become paralyzed ; indeed, they are almost power-

less, as may be known by their emaciated and withered form ; while all the other muscles have been able to retain nearly or quite their full vigor. Now what amount of force, let me ask, applied in the line of that limb, would re-rupture the capsule and return the head of the humerus again to the glenoid cavity ? Indeed, is it not plain that force so applied surely tends to restore the head of the humerus to the glenoid cavity ? My wonder is, how so many old dislocations of the shoulder have been reduced. Many, very many, have failed, altogether failed, in their attempts, by using such means. But how they ever should have succeeded, it is not easy to divine, except we be allowed to suppose that the laceration of the capsule had not become strongly united, and the deltoid and supra-spinatus muscles not completely paralyzed. Under such circumstances, I am fully aware, that, in addition to that degree of extension and counter-extension necessary to bring the head of the humerus out to a line with the plane of the glenoid, to apply force, outwardly, to the shaft, near the head of the bone, would be likely in many instances to effect reduction ; but not in any other instance. It surely must be plain to every surgeon, at all acquainted with the anatomy of the shoulder, the character of its displacements, and the texture of the parts which surround it, that to depend for reduction on the application of force, in the line of the limb, with all the additional force applied transversely which one person could well command, while its capsular ligament is strong and entire (except so far as its lacerated edges are separated by the neck of the bone lying between them), and the two muscles, the deltoid and supra-spinatus, remain powerless, would be but making an attempt, to be defeated in the end. The tendency of force thus acting in the line of the limb, is not, as we have seen in such a case, to elevate the head of that bone, but to depress it ; and it is only by the force which we may be able to apply transversely, that we can even hope to succeed ; and even in this, the greater the traction on the limb, the less must be the influence of that force applied transversely on the head of the bone. With the ordinary strength, therefore, of a healthy capsule, we can hardly expect to succeed by such means.

[To be continued.]

FIBROUS TUMOR OF THE UTERUS.—GASTROTOMY.

[Communicated for the Boston Medical and Surgical Journal.]

THE subject of the following notice is a healthy woman, 43 years of age, the mother of several children. Early in the year 1847 she discovered a small tumor in the left iliac region, for which she consulted her physician, Dr. Duncan, of Shelburne, and during a long period all known means were exhausted to arrest its growth, but without success. Being of spare habit, the superior boundaries of the tumor were easily defined. Its position was central ; it occupied the hypogastric and iliac regions, but inclined a little to the left side. It reached from the pubis to the ensiform appendage nearly, and admitted of pretty free lateral move-

ments. It was globular, symmetrical, smooth and solid. Explored by the natural passages it was found to rest upon the pubis in front and the sacrum behind. In the recumbent posture it could be raised upon the finger and a free impulse communicated to it, but in the erect position it was immovable. Its length might be 8 or 9 inches and breadth 5 or 6, and from repeated examinations by Dr. Duncan and myself, we judged the tumor to spring from the left ovarium, although we were by no means sure it was not uterine. There appeared to be no deviation in respect to the place and condition of the os uteri, nor were the periodical functions of the uterus interrupted by the proximity of the tumor. It was never attended with pain or tenderness; it appeared suddenly and increased rapidly, but beyond this the local or constitutional disturbances were very slight indeed, and we did not hesitate to regard this morbid growth to be non-malignant as yet.

Such being the state of things, our patient was solicitous to take the perilous alternative of an operation. Upon this point no advice was expressed; it was her unbiassed determination. But the dangers and uncertainties of this procedure were faithfully impressed upon her attention, yet she remained firm; and her case appearing as favorable for success as could well be chosen, we consented to attempt the removal of the tumor, and it fell to myself to use the knife.

The sixth day of June, having reference to the menstrual period, was assigned for this object, and as a preliminary measure our patient was requested to take castor oil previously, and to abstain from solid nutriment, a precaution that rendered the bowels flaccid and free from peristaltic movements during the subsequent steps.

Her resolution did not forsake her, and she took her position upon the table with that firmness which true courage only can inspire. With the sanction of several professional friends* an incision was begun upon the left side of the umbilicus, and carried down to the pubis, dividing the integuments and aponeurotic tissues to the peritoneum, which was carefully opened to a like extent, when the summit of the tumor was conspicuous. It was round, polished, solid, and traversed with innumerable vessels that communicated a bright color to its investing membrane. The primary trunks of these vessels were large. In passing the hands into the pelvic cavity to raise the tumor, the intestines escaped with a gush; by this manoeuvre it was ascertained that no adhesions existed, and that the tumor was *without a pedicle*! In attempting to raise the mass, the movement was suddenly arrested by the stretch of the broad uterine ligament, but the uterus was nevertheless fairly dislocated and brought into view, when the true character and relations of the tumor were revealed. It was a solid, fibrous tumor of the uterus.

The difficulties and dangers of the enterprise were now fully apparent. The tumor embraced the entire left half of the uterus, which was enlarged and flattened, and appeared to be imbedded in the tumor rather than to give origin to it. The origin was continued to the left division

* Dr. Duncan; Drs. Stone, Seymour and Reed, of Greenfield; Williams, of Deerfield; Tabor, Willson and Lyon, of Shelburne; and Deane and Puffer, of Colrairie.

of the broad ligament, and thereby the position and movements of the tumor were firmly fixed. To prosecute the operation, the necessity of removing the uterus and its appendages was inevitable; for the possibility of separating the tumor from the uterus appeared to be impossible. The tumor received its nutrient vessels from the uterus, and the difficulties of controlling the hæmorrhage that must result from division, seemed too appalling to be encountered. A brief conference was therefore held, when it was the unanimous expression that the further prosecution of the operation was impracticable; first, because of the danger of hæmorrhage—and second, from collapse or from consecutive inflammation, and we were therefore reduced to the miserable necessity of retracing our steps, and averting the consequences of the mischief we had already done.

The tumor and intestines were therefore returned to their respective cavities, but not without difficulty, and the wound closed and firmly secured by the interrupted suture, long adhesive dressings, compresses, and over all a firm binder. During this distressing period our patient was unconscious of our doings, chloroform having been previously administered by Dr. Seymour. But this agent exerted a pernicious result. Its first inspirations caused paleness and apparent distress, manifested by low moanings, which continued until the patient was put in bed. But no vomitings or nausea occurred, nor as yet much embarrassment of respiration. Right ovary was in a normal state, but left was not seen. The tumor was an ellipse, and its attachment to the uterus corresponded to its inferior focus. Pulse at commencement, 80; it fell to 72. Temperature 78°. No bleeding of note occurred.

The propriety of abandoning the operation was fully justified by subsequent events, for the constitutional disturbances that ensued were severe and threatening. These were due to two distinct causes, the inspiration of chloroform and structural injury. For three hours after our patient was placed in bed, her sufferings were intense. Her countenance was pallid, respiration laborious, mind oppressed, and she constantly uttered plaintive moanings. Yet the pulse did not indicate approaching collapse. It had gone up to 90. She finally fell into sleep, and her breathing then became suspended by intervals of alarming duration. For thirty-six hours it was never above four times per minute, oftener but twice, frequently but once, and sometimes the pause was a minute and a quarter. She was for the most part lethargic, and her muscular system was convulsed with incessant twitchings. Occasionally she might be roused from stupor, but instantly relapsed into insensibility. The extremities were cold. To anticipate supervening inflammation, and especially to avert the issue between the inversed order of respiration and circulation, eighteen ounces of blood were drawn, but no sensible effect was produced upon the respiration. At the end of twelve hours the spasms and insensibility had increased; pulse 100, breathing stertorous, fingers livid and abdomen tympanitic. During the succeeding twenty-four hours our apprehensions were truly painful, the pulse still increasing and breathing diminishing in frequency. But at the termination

of this period, the respirations rose to twelve in the minute, and the intellect began to be relieved of its oppression, and henceforth the respiration became unembarrassed.

But the case was yet encompassed with perils, for severe inflammatory action was now established. From 90, the pulse rapidly increased to 140, and even more. The abdomen was tender and full, and would have burstèd but for vigilant precautions. The tongue was loaded. The severity of the symptoms demanded active treatment, which was of course depletory. But, without narrating particulars, it will be sufficient to say that on the eighth day there were evident signs of amendment, and we dressed the wound for the first time. It had closed, in spite of the distension, except two small spaces at the lower extremity of the incision, from which issued a small quantity of dissolved coagulum. Nothing further particularly occurred to interrupt recovery; in a fortnight, our patient sat up a little, and, finally, although disappointed in our expectations, we were nevertheless unspeakably gratified that the conclusion was no worse.

Greenfield, Oct. 2, 1848.

J. DEANE.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, OCTOBER 11, 1848.

Medical Counsellors' Meeting.—On Wednesday last, Oct. 4th, was held the regular semi-annual meeting of the Counsellors of our State Medical Society, which took place, as usual, at the Masonic Temple, in Boston. Although the weather was unpleasant, several gentlemen were noticed, who came from the interior of the Commonwealth, which shows that there is ample vitality in the Society. Something was said about paying the necessary expenses of distant members, called from home on the Society's business; but it was shown that the matter was already in the hands of a committee. Dr. Jarvis, from a committee, read a long, learned and important memorial on the subject of registration, which was ordered to be presented to the next legislature. It was alarming to hear the fact stated that the average duration of human life in Boston, is only twenty-two years, eight months and eleven days. There was also read the report of a committee on a revision of the by-laws, which was prepared with exceeding care by Dr. Jeffries. By request, it was re-committed, with instructions to make any further needful revision, and print it for distribution among the Counsellors. Finally, it was voted that the meeting should adjourn to the first Wednesday in December.

Tying the Internal Iliac.—Reference was made, last week, to an operation at the Mass. General Hospital, by Dr. H. J. Bigelow, the particulars of which are mainly as follows. The patient, a woman, was stabbed by a brutal husband, in a way to wound the gluteal artery, as nearly as could be ascertained. From various indications, guided by the ear, an aneurismal

tumor was suspected, and Dr. Bigelow very skilfully cut down upon the iliac artery, which was secured with peculiar adroitness. The patient was under the influence of ether, and being perfectly quiet, no interruptions occurred, and only a small quantity of blood escaped. All the symptoms were as favorable as could be expected, till Friday morning last, when it was reported that some peritoneal inflammation existed. Since then, we have not heard, but fully expect a satisfactory result.

Congenital Dislocation of the Head of the Femur.—A reprint of an instructive communication, by J. M. Carnochan, M.D., to the New York Journal of Medicine, on the *Pathology of congenital dislocation of the head of the femur upon the dorsum of the ilium*, illustrated by two plates, has come to our address. It is doing good service to give the pamphlet an extensive circulation. Any practical fact in operative surgery, which has been ascertained by the watchful vigilance of one who has at heart the advancement of the art, is of incalculable value to those who practise the same calling, independent of the benefits immediately conferred on the suffering patient. Every encouragement, therefore, should be extended to those who are willing to contribute to the enlargement of the domain of the surgeon's knowledge, by fortifying him for emergencies, and furnishing precedents and safe rules of practice. We recommend the publication to the special study of students.

Bronchial Comfit.—Fortunately for the vender of this article, it exactly suits a class of consumers who feel it a bounden duty to give certificates of its surprising efficacy. Some of the itinerant vocalists and tragedians, and last, but not the least subservient to the objects of the proprietor, three clergymen, come up manfully to the aid of the comfit—by which they were singularly comforted. Therefore there is nothing like this nostrum for over-wrought throats, as the certificates plainly and unequivocally declare; and now, if the proprietor's fortune is not made by the lucky hit of the reverend gentlemen, then the defect must be imputed to his mis-management, rather than to any imperfection in the construction of their admirably-written recommendations.

An excellent opportunity is presented here, for moralizing on the egregious folly of swallowing down, with a satisfied and greedy relish, all the quack preparations of the day; but it would be a waste of strength. People want to be drugged, and they will spend their money and suffer death voluntarily in gratifying the national propensity to indulge in the luxury of drastic purges and other horrible potations, rather than to buy bread and other necessities and comforts of life. Reason is of little use in such cases. What lady was ever convinced by a logical argument, based on the unchanging truths of physiology, that ligaturing the waist was injurious to health? Not one—nor have all the teachings of the physiologist and physician yet lessened at all the consumption of quack remedies. We are a medicine-taking people: it is an expensive diversion, it is true, with the rich, and a ruinous mania among the poor—but both parties love it, and the never-tiring ingenuity of those devoted to the trade of supplying this morbid national appetite, just keeps pace with a constantly increasing demand.

Homœopathic Medical College.—One more medical institution is to commence operations in Philadelphia forthwith. It is intended exclusively for raising up native homœopaths—the United States having in a great measure been dependent on Germany for the dispensers of infinitesimal doses. On the 16th of October lectures are to begin, the fee for the whole course being \$100, and graduation \$30. Faculty, as follows:—

Jacob Jeanes, M.D., Professor of the Principles and Practice of Medicine. Caleb B. Matthews, M.D., Professor of Materia Medica. Walter Williamson, M.D., Professor of Midwifery and the Diseases of Women and Children. Francis Sims, M.D., Professor of Surgery. Samuel Freedley, M.D., Professor of Botany. Matthew Semple, M.D., Professor of Chemistry. William A. Gardiner, M.D., Professor of Anatomy. The Chairs of Physiology and Pathology remain to be filled. Clinical instruction in Medicine and Surgery will be given at the College.

Reform of Manners among Physicians.—Singular as it may appear, one of the contributors to the Homœopathic Journal cries out lustily for a reformation of manners among physicians. He must have exclusive reference to his own sect of practitioners, because the gross evil of which he complains has not been mentioned as existing in the allopathic ranks. It is lamentable that brethren of the same school cannot dwell together in harmony. Some of the high potencies look down, we fear, with contempt on disciples of small calibre. Alas, there is no perfection even in homœopathy.

Mode of Reducing Dislocations of the Humerus.—Mr. Morgan describes as follows, in the Provincial Journal, a mode for effecting the above object, which he states to be found convenient and useful in the practice of the Bristol Infirmary.—*London Lancet.*

"Without any preliminary treatment, the patient is seated sideways on a firm chair, with his arm hanging over the back, which is well padded; one end of a double or reel-towel is passed through the other end, so as to form a noose, which is applied to the arm just above the elbow. The loose, depending part of the towel forms a stirrup, into which the surgeon places his foot, and gradually brings his whole weight to bear on the towel, as an extending power. One or two assistants are useful to press back the acromion, and keep the patient firmly in his seat. The reduction is effected almost immediately; and if due precaution is observed in properly padding the chair, and the arm where the towel is applied, little or no pain is felt, nor any subsequent inconvenience from the pressure."

Medical Miscellany.—Cholera has appeared in the Grecian Islands.—Mr. Otis Clapp, of Boston, proposes to publish a Quarterly Homœopathic Journal, commencing Jan., 1849, conducted by A. C. Becker, M.D., provided subscribers enough come forward to warrant the undertaking.—Tobacco costs the people of the United States \$16,000,000 annually. Of this sum, \$9,000,000 are dissipated in the smoke of Spanish cigars.—It is stated in Capt. Allen's narrative of the Niger expedition, that those infants among the Ibus, which happen to have the front teeth of the upper jaw cut first, are put to death, as they are regarded as hated by the Fe-

tishes. A female having twins suffers a long exclusion from society, and the children are invariably exposed to wild animals, and the mother ever after viewed with suspicion and disgust, as an object of Fetish wrath.—Letters from Smyrna, of August 27, state that the number of persons attacked with cholera was about 200 per day, and that the deaths were from 80 to 100. The victims were principally the poorest classes of Turks and Jews.—The Prussian Tom Thumb was interred at Oldham, Eng., in presence of thousands of spectators. He was 65 years of age, and when laid in his coffin measured exactly three feet in length.—In Lyons, suicides are following each other with fearful rapidity. Four occurred in a single street in one day.—It is contended in England that the cholera does not attack persons who live near breweries or mineral springs, in consequence of the counter influence of carbonic acid gas evolved there. All the watering places with springs that emit this gas escaped the pestilence in Germany, Spain and England. Balston and Saratoga also escaped.—The small-pox has made its appearance in some sections of the city of Washington. The Board of Health recommend that, as far as practicable, all the infected portions of the city should be avoided.—The newspapers state, as a fact, that a physician in Washington, in prescribing for a hotel waiter, wrote fifteen grains of *morphine*, intending to write *quinine*. The patient was found dead in his bed.—Cholera has appeared at Trieste. It is on the increase in Russia. Whilst the political world in Europe is convulsed, this disease seems to be advancing with steady strides. There have been several cases in Paris; at Berlin 1800 cases, and at Hamburg 280. It was raging at Constantinople, Aleppo and Damascus, nearly decimating the inhabitants.—A medical student in China, a native, who sought the degree of *doctor*, made an expression, in his thesis, that was thought to reflect on the emperor improperly, for which he was sentenced to receive a hundred lashes. At this, about 5,000 students collected, disarmed the soldiers and presented themselves before his celestial majesty, who, on learning the nature of the case, revoked the decree.—Dr. S. S. Purple, who has for the last year and a half assisted Dr. Charles A. Lee in editing the New York Journal of Medicine and the Collateral Sciences, is announced as his successor in the editorial chair.—The number of medical men in London is stated to be greater than the butchers in that city, and nearly as great as the bakers.

TO CORRESPONDENTS.—Dr. Chapin's case of Acute Ileitis, and Dr. Castle's case of Lithotomy, have been received.

MARRIED.—Charles H. Wetmore, M.D., of Lebanon, Conn., to Miss L. Taylor.

DIED.—In Newburyport, Dr. George W. Goodwin, 62.—At Newton Lower Falls, Mass., Ezra Nichols, M.D., 58.—In Jackson Co., Tenn., Dr. Elias W. Napier, who by will emancipated twenty-eight slaves.—At Rome, Dr. J. L. Martin, U. S. Consul at that city.

Report of Deaths in Boston—for the week ending Oct. 7, 75.—Males, 42—females, 33.—Of consumption, 9—disease of the bowels, 20—dysentery, 21—cholera infantum, 1—typhus fever, 2—lung fever, 1—brain fever, 1—slow fever, 2—infantile, 4—teething, 1—inflammation of the lungs, 2—delirium tremens, 1—convulsions, 1—rancer, 1—croup, 1—dropsy on the brain, 1—scarlet fever, 1—inflammation of the brain, 1—child-bed, 1—old age, 1—erysipelas, 1—intemperance, 1.

Under 5 years, 37—between 5 and 20 years, 5—between 20 and 40 years, 14—between 40 and 60 years, 13—over 60 years, 6.

Neglect of the Medical Corps of our Army and Navy.—Why have there been no promotions in the Medical profession connected with our Army and Navy in the recent campaigns in Mexico? We have looked in vain for the first surgeon's name, over the long list of promotions among the regular and volunteer troops. There have been created some 500 brevet-commissions during the last two years—indeed, scarcely an officer of our army, it matters not in what capacity he has served, so he was not in the medical staff, who has not been promoted. We read of most onerous duties faithfully performed during the whole war, of gallant services during engagements with the enemy, of several, many sad deaths occurring among the surgeons of the Army and Navy, but not of a single promotion. It is thus ever with the medical profession; whether in civil or military life, we must toil on, toil ever—labor, day and night, before, during and after a battle—and do this without distinction or reward. The soldier who performs one daring deed, is rapidly advanced and adequately requited; but the surgeon, equally self-sacrificing, who renders duties far more arduous, is seldom noticed—never promoted. We are familiar with the heroes of every battle fought in Mexico; but, with the occasional or general commendation at the close of an officer's report, we find no further notice of that highly respectable corps of medical men, who have rendered most efficient service to their country, and who have saved many a valuable life. Again we ask the proper authorities, why there have been no promotions among the Surgeons of our Army and Navy? Have they alone failed in their duty? if not, why are they alone neglected?—*Southern Med. and Surg. Journal.*

The Value of Tears in the Prognosis of the Diseases of Children.—Trousseau affirms that, when a child cries its disease is not grave; that when it does not cry, its disease is very grave. This he thinks may be regarded as an aphorism. If the child cries, it will recover; if it does not cry, it is very seriously ill. Although there are exceptions to this rule, still they are so rare that the physician should hold it in very sacred esteem.

If you are called to a child who up to that time had cried, and who does not cry when you vex it, if its eyes become suddenly dry and sunken in the orbits, look out for a very grave disease of some kind; you will rarely be deceived, unless, as sometimes happens, the child never cries naturally, and there are those of this kind. On the other hand, when you have detected souffle and dulness in a chest, when you have discovered hypertrophy of the liver or spleen, tympanitis; if you see the child shed a few tears, let your prognosis be favorable, it is the sign of early convalescence in almost every instance. This sign is of value until the seventh year, when it ceases to be important. In the two first years, and especially in the first, it is almost invariably true. After the first year it is less to be relied on, and gradually diminishes in worth every year till the seventh, when, as I have said, it becomes valueless. In serious operations on children, not for external diseases, but for affections, which although local, give rise to very grave symptoms, as croup, for instance, I have never seen a child cry under the bistoury. The little beings suffer, but without weeping; and when, some days after, I see them shedding tears, I rejoice; and when they do not weep, I predict an unfortunate termination, which is nearly always realized.—*DR. YANDELL, in West. Med. Journal.*